

Greenwald, Peter 2008 A

Dr. Peter Greenwald Oral History 2008 A

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Division of Cancer Prevention Oral History Project

Interview with Dr. Peter Greenwald

October 17, 2008

History Associates Incorporated

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Biographical Statement

In 1981 Peter Greenwald, M.D., Dr. P.H., created the Division of Cancer Prevention (DCP) at the National Cancer Institute (NCI), U.S. National Institutes of Health (NIH), in Bethesda, MD, and had led its activities for more than three decades as division director. A native of Newburgh, New York, Dr. Greenwald attended Colgate University, where he received a degree in chemistry and physics. After earning a Medical Doctorate from Upstate Medical Center in Syracuse in 1961, Dr. Greenwald interned at Los Angeles County Hospital. In order to fulfill his draft obligations, he joined the Public Health Service in 1962 at the Communicable Disease Center in Atlanta (now the Center for Disease Control and Prevention). Dr. Greenwald completed a residency at Boston City Hospital in 1964 and then earned a Masters in Public Health and Doctorate of Public Health in Cancer Epidemiology from the Harvard School of Public Health.

Before joining the Division of Cancer Prevention and Control in 1981, he served as the Director of the Division of Epidemiology of the New York State Health Department.

After becoming Director of the Division of Cancer Prevention and Control, Dr. Greenwald proactively set specific goals and guidelines in order to significantly reduce cancer incidence and mortality. While reorganizing the Division, Dr. Greenwald also served as Editor in Chief of the Journal of the National Cancer Institute from 1981 to 1987. While at the Division of Cancer Prevention, he has promoted, among other things, the development of Tobacco Control Programs, Chemoprevention Agents, a Community-Based Clinical Oncology Program, large scale Chemoprevention and Early Detection Clinical Trials, and the expansion of the cancer prevention and control public health infrastructure.

Shortly after his appointment to head the newly reorganized DCP in 1997, Dr. Greenwald received the Outstanding Research Award from the American Institute for Cancer Research and the Distinguished Service Award from the American Cancer Society.

Interview Synopses

In interview one, Dr. Greenwald traced education and employment activities that prepared him for his work at NCI. He noted a summer fellowship in Iran during medical school that permitted him to learn about many conditions rarely seen in the United States, particularly cutaneous anthrax. He emphasized his training and experiences as an Epidemic Intelligence Service (EIS) officer at what is now called the Centers for Disease Control and Prevention (CDC), which advocated assessing a public health problem and addressing it with action. This contrasted with his postgraduate public health training at Harvard University, which emphasized rigorous, evidence-based methods for statistical research on chronic diseases, especially cancer. He described his decision to unite the two training philosophies by accepting in 1968 a position as Director of the Cancer Control Bureau, New York State Department of Health and gave examples of his work before coming to NCI in 1981.

Interview two described how he created a new Division of Cancer Prevention and Control (DCPC) at NCI and on the programs DCPC sponsored. Dr. Greenwald outlined the

philosophical concepts of cancer "prevention," cancer "control," and cancer "etiology" and how he implemented evidence-based research on cancer prevention and control. Specific programs that he discussed during this interview include the Smoking and Tobacco Control, Diet and Cancer, Chemoprevention, and the Community Clinical Oncology Program, with subgroups emphasizing the needs of minority populations to address health disparities. Two particular clinical trials discussed in detail were the National Surgical Adjuvant Breast and Bowel Project and the Prostate Cancer Prevention Trial.

Interview three continued the discussion of major clinical trials supported by Dr. Greenwald's division and also detailed internal NCI administrative processes by which resources were allocated to different divisions. The difficulties of nutritional research for cancer prevention and of defining valid biomarkers for indicating stages in the process of carcinogenesis were also detailed. Dr. Greenwald commented on the priorities of different NCI directors and the effect they had on his efforts to identify risk reduction for specific cancers through clinical trials.

National Cancer Institute

Division of Cancer Prevention Oral History Project Interview with Peter Greenwald

Conducted on October 17, 2008, by Victoria Harden

VH: This is the first in a series of interviews with Dr. Peter Greenwald, the Director of the Division of Cancer Prevention, National Cancer Institute [NCI], on October 17th, 2008, in his office in Bethesda, Maryland, about his career at the National Cancer Institute, National Institutes of Health [NIH]. The interviewer is Victoria Harden. Dr. Greenwald, would you begin by stating your full name, and that you're aware that we are recording this interview, and that it's done with your permission?

PG: Yes. My name is Peter Greenwald. I'm Director of the Division of Cancer Prevention at the National Cancer Institute. I fully approve of this being recorded and appreciate your efforts in doing it.

VH: Thank you. You were born in Newburgh, New York, on November 7th, 1936. Would you tell me some about your family and your early education through high school?

PG: Yes. Newburgh is a small city. Its population was about 25,000 to 30,000, and it is located 60 miles north of New York City on the west side of Hudson River. I was actually born in the house where we lived. An aunt helped my mother deliver me; then they put me in a box on the radiator. I've heard that story many times. I have one older brother, very active, outgoing, I'll mention a little later; and two younger sisters. Three of us, my older brother Michael [Michael Greenwald], sister Ruth [Ruth Kaplan], and I were close in age. We had the same mother and father. We lived in Newburgh until about my fifth grade of elementary school. Ruth was outgoing. I always admired her ability to handle any social situation. Ann [Ann Horton], my youngest sister, is about 10 years younger than me. I'll come to her later.

VH: Let me interrupt you and ask you to state your parents' names.

PG: My father, Louis Greenwald, was a pianist who had studied at Julliard. My daughter Laura [Laura Greenwald Frommer] has been researching his past and has a scrapbook of newspaper articles from 1927 to 1928 when my dad toured the world with Efrem Zimbalist. Had wonderful reviews. He accompanied Zimbalist throughout many parts of the Far East. He was very bright, very quick-witted, but had a sense of humor that was cutting with some people, unfortunately, like my sister Ruth's boyfriends when she was a teenager. My three siblings and I spent much of our time outdoors--riding our bikes, collecting butterflies, doing all sorts of things. Went to the local schools. My mother was a –

VH: Her name?

PG: Pearl Reingold was her maiden name, Greenwald her married name. She and my dad met in New York City through a cousin of my mother's. The early part of my life was a happy time. My mother was very kind and devoted. We spent a lot of time outdoors.

We did things like – I don't think much of the National Rifle Association, but the NRA taught how to put rifles over fences and shoot. I would never shoot at animals, but my brother would pick them off, and Ann was a sharpshooter, hitting bottles as they bobbed in the Hudson River. We had many hobbies. Michael had the most. Right after World War II, my father earned a good living. He often played tennis with a woman named Frances Breed, who could trace her ancestry to the puritans who came to the Massachusetts Bay Colony in 1630. Frances was a wonderful lady, very well educated. She had a great influence on me, partly because she knew about colleges and how to steer us to first rate education. My father, while he went to Julliard, didn't really know about other colleges. And my mother was a very kind and sweet, very pretty high school educated woman. About the fifth grade, we moved to New York. My mother and father had been not getting along that well, for reasons I never understood, but I think Dad fooled around with Frances. My parents got divorced, and my father married Frances Breed. A year or two later, my sister Ann was born. Ann, Ruth, and I are still very close. The sibling relationship is as if we had the same mother and father, but I am ten years older than Ann and we lived in different cities after I left for college. Michael means well, but he has more difficulty keeping in contact.

In New York, we lived on Central Park West and 93rd. I belonged to a gang. We protected our turf. Here is one example: some kids from uptown tried to take our field in Central Park where we played ball. I whacked one kid over the shins with a baseball bat, and the invaders took off. I'd walk through the park at night because I took painting lessons – actually at a YMHA [Young Men's Hebrew Association] on the east side of the park. I'd walk home around the reservoir. My parents would worry about this, but it was before gangs carried real guns and knives. They were tough but not dangerous. I went to a junior high called Joan of Arc on 93rd Street, which later, I understand, was cited for having the most drugs of all high schools in New York City. At the time I was there, it was okay. And the school tracked the kids who were better in math and science, so I got a good education.

For the 9th grade, I went to a private school, receiving a scholarship from the principal who lived on the same floor of our apartment house. It was called Bentley School on 86th Street. It was a personalized education, helping me to get over shyness. Eleanor Roosevelt spoke at the graduation. Then we moved back to Newburgh for the two years before I left for college. The Newburgh schools were good because New York State had Regents Exams that were required for all students in order to graduate. But the Newburgh schools were behind where I was at that time, so I skipped the 11th grade and went from the 10th into the 12th. I started college at age 16 at Colgate University.

VH: When did you decide to go to Colgate?

PG: I applied at the usual time during my senior year of high school. Ann's mother, Frances, knew of Colgate because a number of her cousins went there. At that point, it was a boys' school, but it was an excellent college. My brother was at nearby Cornell. At Colgate, I majored in chemistry and physics.

VH: You indicated that your step-mother was a very important influence in terms of where you chose to go to college. Would you elaborate a bit?

PG: Yes. She was scholarly and knew a great deal about academics. My mother, who, when we moved to New York, moved into the same apartment building because she wasn't going to be apart from us, would talk occasionally with Frances. Their relationship wasn't exactly warm, but they would talk to each other. Frances had gone to Barnard.

She was a violinist, a moderately talented violinist, but well-educated and worldly, exposing us to ideas that we might not otherwise have thought about.

VH: Did any of your family members encourage you to look at medicine? Or did that come later?

PG: That came later. No one encouraged me to go into medicine.

VH: Who were your heroes as a boy?

PG: Gosh. I don't really remember many. I knew the Yankee baseball players, like DiMaggio. I don't remember any heroes in science. I kind of liked math. On every Regents exam I took in high school, I always got 100 percent, but it wasn't a big deal. I looked up to my brother a lot. My brother was almost like an engineer even when he was little. For example, we had a seaplane base in Newburgh, and without telling my parents, he worked there in exchange for flying lessons. He was a good pilot by the time he was in high school, and eventually he went on to fly gliders and teach flying. He has all sorts of planes now. He learned to take engines apart. He knew everything about cameras and power tools. Other than that, I knew of FDR [President Franklin D. Roosevelt]. He came to our town a few times because Hyde Park, New York, isn't far away, and he drove in an open car down Broadway.

VH: Did you have any high school teachers who were especially influential?

PG: I remember my geometry teacher, a tall fellow named William Brown. He knew I was getting bored in class, so he let me teach the class to keep my interest alive. I remember Reginald Shaw, my chemistry teacher, and Robert Fowler, the physics teacher, who built a strong math and science program and later became principal of the high school. But I started in the 10th grade in Newburgh, and then I skipped to the 12th grade, so I didn't really have a class to identify with. I only remember one student from my class, Barbara Cohen [Barbara Cohen Levey]. Now she's assistant vice-chancellor for biomedical sciences at UCLA [University of California, Los Angeles]. She had taken piano lessons from my dad and lived a block and a half away. She was a little older than me—she was Michael's age. She went to Cornell, and then became a classmate of mine at medical school, already going out with her future husband. I would tease her. She'd get annoyed with me if I went to a party on the day before a test because she sort of felt responsible. And I'd tell other students that that she used to baby-sit for me when I was a toddler, since she was older.

VH: I see. All right, let's go back to Colgate now. You double majored in chemistry and physics.

PG: Yes.

VH: Tell me about what, at Colgate, you see as important to the development of your career.

PG: Scientifically, Colgate was very solid. Socially, it wasn't too great. There was anti-Semitism, rooted in the fraternity system. And fraternities were the center of campus social life. I never joined and don't think they wanted me, because I was more independent than "fraternal." But the courses at Colgate were excellent. The intramural sports were good. I like living outdoors. It's very outdoors in Hamilton, New York, where Colgate is located. I enjoyed the Outing Club. I liked the science, and I liked the outdoor activities – the environment, but I have only a very few friends from there. I have many more friends from medical school, although I rarely see them.

VH: I'm curious as to whether the facts that you skipped a grade and that you were younger than most of the other students, had any influence on the whole –

PG: I don't think it did at Colgate. It may have during the clinical years in medical school because I looked very young, but I don't recall its being a problem in college. I was into math and physics. The biggest problem I had, which hurt my grades in the last year, was trying to take two courses at once. I went to two professors and wanted to take two courses that had labs in the same afternoon, which they approved; then they both expected me to be in their labs, and I couldn't.

Even into my senior year, I was not thinking of medicine. I was thinking of physics, maybe. But you had to apply to medical school earlier in the year before you would apply for, say, a Ph.D. And so I applied for medical school, and I got in. The first place to accept me was Albany. We didn't think about going far away from home like people do today. I was an upstate New Yorker. I think the reason I got into Albany quickly was that I had built an oscilloscope. When I went for the interview, the deputy dean who interviewed me was very much into electronics. When he heard that I had built an oscilloscope, it seemed right up his alley. I ended up going to the Upstate Medical Center in Syracuse, a part of the State University of New York.

VH: Let's move on to medical school. Tell me about your training. I'm particularly interested in what sparked your interest in epidemiology and cancer.

PG: The medical students were a close knit group. This was one of the strongest periods in terms of quality, for the Upstate Medical Center. There were some well-known people the year before me, like Sam Their [Samuel O. Their, M.D.], who was later president of Mass General [Massachusetts General Hospital]. So, in general, it was a solid medical school. They were training a lot of clinical doctors. I remember the first baby I delivered as a third-year medical student. A young lady came in, the obstetrician didn't show up in time, but I was there with the nurse, so I delivered the baby, which was fine, and then the woman said, "Hello, Peter." It turned out she was the wife of another medical student!

That's why I remember it; the delivery was easy.

Several faculty influenced me. One was Harry Feldman [Harry A. Feldman, M.D.], who did childhood vaccine studies. There was a low income housing community right behind the medical school, and it served as his community lab. He would test for the effectiveness of vaccines in these children. This may have started my interest in epidemiology.

The other person who influenced me in an even more important way, was a pediatric infectious diseases expert named Paul Wehrle [Paul Wehrle, M.D.]. Between my third and fourth years of medical school--in the spring, maybe April--I saw an ad for a fellowship that would take you to any developing country in the world. It was sponsored by the American Association of Medical Colleges and paid for by a drug company, SmithKline. But there were only had two weeks left before the deadline for submitting an application, when I saw the ad. I talked about this to Paul Wehrle, with whom I was very friendly, and he said, "Let me call --" his friend. His friend was James Halstead [James Addison Halsted, M.D.], a gastroenterologist who was FDR's son-in-law. He had married Anna Roosevelt. Halstead was taking a two-year stint in Shiraz, Iran, where there's a medical school which is largely, at least at that time, related more to English and U.S. medicine, than Tehran, which was more closely related to French medicine. Halstead knew of a missionary doctor who visited the villages near Shiraz as well as running his own little hospital. I said, "Oh, could you arrange for me to spend the summer in a village?" He did; I got the fellowship. I went to Iran, and I actually spent a few days at the medical school. What impressed me was that the medical students-- young men and women, dressed just like us. No Islamic head coverings, nothing like that. Society appeared very open under the Shah. I had trouble picturing how the educated Persians tolerated the changes after the Islamic revolution occurred.

It was a wonderful experience in that village, named Qasroddasht. The village owner was a gypsy, a tough guy, whose tribe would live in tents surrounding this village six months of a year, then move elsewhere for the other six months. He had built what they called as a hospital. It was a row of rooms made out of dirt which hardened, almost like brick. There was a midwife there and her husband, who was sort of a medical aide. This couple ran the hospital. The missionary doctor would visit one day a month. I was the new "doctor," but I didn't know much. I'd had just three years of medical school. My first patient had a scorpion sting on his finger, which I had never seen, so I treated his pain with a local anesthetic, which just seemed logical. There also was a lady with anthrax. On the left side of her lip, between her chin and cheek, she had had a cutaneous anthrax lesion. The local villagers had treated it before I saw her. The people who took care of her called it a "boil that must be boiled." They put this clay donut--clay molded with a hole in the middle of it--over the lesion and poured in boiling oil, so what she really had was a huge burn, but it cured the anthrax. And then I noticed anthrax in some of the sheep and goats. There were lots of intestinal worms, parasites, some cases in which they had caused obstruction. I had to send the patients with obstructions to Shiraz for care. The village people came from all over thinking that because I was a foreign doctor, I must be good. But I felt like I didn't know anything.

VH: But you were getting all sorts of experience.

PG: Oh, yes. There were a lot of conditions, including several diabetics who were taking insulin. Usually, you could figure it out pretty easily. There were lots of drugs there, which were provided by the Point Four Program [a program for economic aid to poor countries, 1950-53], so they were stocked with drugs that often required more sophisticated medical knowledge than was available, so they were not used.

VH: After you finished medical school, in 1961 you went to Los Angeles for your internship. Tell me about that year. What was important?

PG: My internship was at L.A. County Hospital, a large charity hospital with lots of patients with advanced disease, multiple diseases, trauma. I interned in one of the last years when young doctors took rotating internships, and I lived on the hospital grounds for most of the year. So I spent a lot of time on surgery, obstetrics, orthopedics, pediatrics, psychiatry, as well as internal medicine services. I thought that was good. I learned a lot about multiple specialties which doctors now miss. Now they get so specialized, they often don't have a good sense of the scope of medicine and how to treat a whole person.

The emergency room was like a combat zone. There was even one guy with a ascites liver problem—a swollen, fluid-filled abdomen—who came wearing a bandana on his head. The stressed admitting intern thought he was a pregnant female and sent him to obstetrics.

L.A. County Hospital had a separate floor for diabetics. The hospital was staffed by two medical universities—the University of Southern California [USC] and Loma Linda [Loma Linda University School of Medicine]. Paul Wehrle had actually gone to be a professor at USC the same year I went to L.A. County. It didn't affect my choice. It was just by coincidence that we went to the same place. This was a terrific place given my interests, because on the L.A. Hospital grounds was a communicable disease hospital. By law, a whole category of communicable diseases, diagnosed anywhere in L.A. County, had to go to this hospital. So I was able to see tetanus and all sorts of other rare infectious diseases that you don't usually see during your internship. Some doctors don't see these in their entire professional lives. I'd see many people with rare communicable diseases at all stages of advancement.

VH: Was there anybody else from your medical school class who was doing an internship with you?

PG: Yes. Six medical school classmates went to L.A., some to the UCLA Veteran's Hospital and others to L.A. County where I was. And we kept close contact all that time. L.A. County Hospital had a large contingent of interns overall—around 160.

VH: Was there anything other than the infectious diseases hospital that you think was particularly important for your later career during that period?

PG: When I was thinking what I wanted to do, there were two major forces. One was my brother Michael, who was two years behind me in medical school, even though he was older because he'd been in the Army for three years, between his second and third years at Cornell. Michael was very mechanical, and he had figured out from income tax data that orthopedics was high paying and would allow him to live the lifestyle to which he wanted to become accustomed. Michael suggested that he, a friend, and I set up a practice for two—three doctors running a two-doctor practice. When you figured out all the income tax and everything, each of us could take one month off each season to go skiing, or go surfing in Florida or the like. Therefore, I lined up a fellowship to do bone research for a year and then orthopedics at UCLA, after completing my internship. I thought of that seriously, but decided it wasn't for me.

Also at that time, there was still a physician draft. The Vietnam War had started under Kennedy [President John F. Kennedy], but this was before the period that's now called the "Vietnam era." Since I wasn't really sure what I wanted to do, I decided to fulfill my two-year draft obligation in the military. I had all the papers to be a flight surgeon. A flight surgeon is, basically, a family doctor for the Air Force, allowing you to fly around and see the world. I thought, "Oh, might as well see the world, enjoy myself."

Just at the time I was filling out the papers to join the Air Force, I got this phone call from Paul Wehrle. In addition to being the lead infectious disease expert at L.A. County Hospital, Paul had strong ties to CDC [Centers for Disease Control and Prevention], then called the Communicable Disease Center. He asked if I wouldn't want to go to CDC rather than joining the military. During the Vietnam War, the programs at CDC and NIH sometimes were referred to as the "Yellow Beret" programs. Both were a way to fulfill your draft obligation. So I said, "Okay, I'll come to CDC in Atlanta and consider it." In April, CDC has a week where they recruit the young EIS [Epidemic Intelligence Service] officers. And so I went to visit the CDC. It looked terrific. I joined CDC rather than going into the military. It was a wonderful experience, which we can talk about.

VH: Just by way of information, did you ever consider the NIH at that point, or did anybody bring it up to you?

PG: No. No one raised it. It never entered my mind. I didn't know anything about NIH.

VH: So let's move you to Atlanta from Los Angeles. You would have been commissioned in the Public Health Service as a – what's the first rank? Assistant surgeon?

PG: Yes. Assistant surgeon, with rank equivalent to Navy ranks.

VH: You arrived in Atlanta in July 1962. Tell me about your training.

PG: I took the summer EIS course which Alex Langmuir [Alexander Langmuir, M.D.] led. The two deputies under Alex, D.A. Henderson [Donald A. Henderson, M.D.] and Phil Brockman [Philip Brockman, M.D.], contributed, and there was a strong statistical group. They taught us practical field epidemiology. If you are faced with an epidemic, an outbreak, what do you do? How do you figure out what's going on? And they ingrained, what Alex – who later became a close friend–called “shoe leather epidemiology.” Shoe leather epidemiology meant that you go right to the scene and deal with the epidemic.

Alex's view was that if someone like a local health officer or state health commissioner called, you sent a young epidemiologist right out same day or next day. It seemed different at NIH. When called about infectious disease outbreaks, we thought NIH scientists would look at one and say, "Does that fit my interests?" and often would not respond. At CDC you sort of got this feeling of arrogance from NIH. CDC was dedicated to responding. And I felt they took the infectious disease lead for the country away from NIH.

That lasted until AIDS [acquired immunodeficiency syndrome] came along about 1981. CDC grew because of Langmuir's approach to being immediately responsive.

Also I feel that there was an attitude that as an epidemiologist, you're there to figure out and deal with public health problems. It's not just knowing, but it's also doing—taking public health measures to address the problem. It concerns me today that the reward system (tenure and promotions) often is based on publications rather than public benefit. It's not the scientist's responsibility to go beyond publication in a peer reviewed journal. Publications count the most for glory in the scientific community, especially basic scientific discovery as opposed to applied.

VH: Several people in other interviews have told me that, in the decades after World War II, there was a major shift in the way people thought about their careers. At NIH, they shifted from thinking of themselves as public health people primarily, to thinking of themselves as researchers, academic researchers primarily. You would agree with that then?

PG: I think so. I don't know a lot about the people at NIH before I arrived in 1981, except the ones who were more closely related to my field, but I did sense that, especially when I started at NCI. In 1979, Art Upton [Arthur Upton, M.D.] was NCI director and invited me to talk about a job at NCI. Not because of Upton, but I sensed at that time that anything having to do with public service or public health was being pushed aside in the corner and not supported with resources or with encouraging the best people. And I think that problem exists until today. It's not with all of the institutes, but it is with NCI.

VH: Were there any other people whom you met when you were in EIS training who have stayed colleagues and friends and supporters?

PG: Yes. After my medical training, I went to New York State, and later after joining NCI, I recruited two EIS officers from CDC. While associated with CDC, I worked closely for 2-3 months with Bill Foege [William Foege, M.D.] and two others, Drs. Pierce Gardener [Pierce Gardner, M.D.], who's here at the Fogarty [John E. Fogarty International Center, NIH], and Ron Roberto [Ronald R. Roberto, M.D.], who I think is in a public health position in Berkeley, California. The four of us did a vanguard study for the smallpox eradication program in the Tongan Islands in the mid Pacific.

VH: Tell me about that.

PG: D.A. Henderson provided the leadership. Since smallpox is only acquired and transmitted by people, not animal vectors, and since we had a tremendously effective vaccine, it was thought feasible to eradicate smallpox from the world. But first we had to do some testing. At about that time, the crown prince of Tonga (Viliami Tungi), had come through CDC – a Polynesian fellow who was well educated in Australia. He felt that it would be good to do a smallpox vaccine trial in his country because it would help educate his physicians. He could rationalize doing it because the next island over, Fiji, had a lot of people who were immigrants from India where there was smallpox, and they might expose visiting Tongans. We set up to test freeze-dried smallpox vaccine with the idea of finding out if we could reconstitute it—basically adding water--and whether it would still be effective in the tropics. We also wanted to work out what an appropriate dose would be, and we were testing a modified jet-injector gun. The Army had developed this gun, which ran on electricity. You pulled a trigger and it cocked a piston. You pulled another and it shot a thin jet of fluid through the skin without the need of a needle.

VH: If I remember correctly, you have to inject smallpox vaccine into the skin rather than injecting it into a muscle or blood vessel.

PG: Correct. In and not through the skin. To achieve that, the nozzle was modified to be at an angle. This created a little bubble of vaccine in the skin like the old TB [tuberculosis] tests. The vaccine would be in the skin, not through, to give better immunity. The electric part of the jet injector was replaced by a hydraulic foot pedal so you could cock the piston with no electric power. We divided the doses – if you took the little vial, the drop that doctors in the United States used as one dose, we were testing a 10th, a 50th and a 100th of that dose.

VH: And what did you find?

PG: We found that with the 1/50th, there was very, very good immunity, and that you could use a freeze dried vaccine.

And the work was fun. We also had generators to show movies to kids who had never seen a movie. We had Polaroid cameras. They loved us. I went to a northern island (which means more toward the equator, because the Tongan Islands are south of the equator) named Vava'u, a beautiful volcanic island with a nice harbor. I did all of the vaccinations on Vava'u myself, with some help from local Tongan health professionals. It all went well except that one lady died after I left. After you are vaccinated with smallpox vaccine, you get an open sore in 10 days or so. She had rubbed dirty leaves into that sore, and she got tetanus. She died of tetanus.

The other interesting story had to do with the brother of the Crown Prince who started the project. Prince Uluvalu Tu'ipelehake was his name. He took one of the two ships owned by the country, kind of a tugboat, and was going up to a little island way in the north nicknamed Tin Can Island (actually Nivafo'ou). The reason it was called Tin Can Island was because it has no harbor, just a rough-edged coast line. Ships would go by about once a month, but it wouldn't try to dock because of the shoreline. The natives would go out in an outrigger canoe. The mail would be put in a tin can and dropped in the water (or lowered with a stick), and they'd pick it up.

The island was volcanic--doughnut shaped with a lake in the center. This visit was the first by royalty in two years, so the residents planned big parties. But unlike his brother (the crown prince), Tu'ipelehake was not a great sailor, and he sometimes would get seasick. As his ship was passing Vava'u, where I was working, it pulled in so I could give them some medicine for seasickness, which I did, but I also said, "We've got to vaccinate the people on Tin Can Island." I hopped on the boat with them. Prince Tu'ipelehake had a son, who was six years old. I played with the bright young fellow a lot. We swam in the volcanic lake.

After I returned from Tonga to Columbus, Ohio, where I was assigned as a CDC officer, I saw a short paragraph, maybe four or five sentences, saying, "Tin Can Island bombed with flu vaccine." What had happened was this: Tin Can Island didn't have any doctors, just an old retired pharmacist. As is typical on a remote island, people have little immunity. The six-year old I mentioned had a cold, and I imagine the cold swept through the island. I don't believe it was the flu, but a cold virus. But I could just imagine them blaming the upper respiratory epidemic that swept the island on me and the smallpox vaccine.

When the Tongans got in medical trouble, they would call New Zealand. So when this virus swept through Tin Can Island, the New Zealand Air Force or Coast Guard came and parachuted some flu vaccine down to them, and they gave it to all the residents. The press called it "bombing" the island.

VH: Not that it stopped the common cold epidemic.

PG: No. It wouldn't have helped.

VH: When you returned from Tonga, you were on an assignment from the CDC to the Ohio Department of Health in Columbus. Tell me about your experiences there.

PG: I was assigned to work with a Dr. Winslow Bashe [Winslow Bashe, M.D.], a pediatrician who ran the communicable disease department for the Ohio Department of Health. Later, he went to the University of Cincinnati as a professor. Harold Decker [Harold A. Decker, M.D.] replaced him. They were both very capable mentors. Ohio had a very good health department at that time.

Let me give you an example of my work in Ohio. Polio vaccine was being used.

The Sabin vaccine [live virus polio vaccine developed by Albert B. Sabin, M.D.] had been recently approved. I was working a bit with Fred Robbins [Nobel laureate Frederick C. Robbins, M.D.], who was at Cleveland City Hospital, Fred Robbins, and one of his associates, Martha Lepow [Martha L. Lepow, M.D.], another virologist. I helped them with studies. One was a survey of polio immunity in Cleveland. We knocked on doors and drew blood from one to four-year olds, to check their immunity against polio. We had no problem any place except Shaker Heights, where the mothers wanted to call their own doctors. The trickiest thing was if you couldn't draw blood from a vein in the child's arm, you drew it from the external jugular vein on their neck with their mothers watching. I actually did better than most of the blood drawing surveyors in getting the mothers to agree to the study, possibly because I was a young doctor.

In Ohio, there were several cases of vaccine-associated polio. I investigated an 18-month old child who lived in an apartment with an adjacent apartment with its door catty-corner to the door of his apartment. Another child in the adjacent apartment had been vaccinated with the Sabin vaccine about three weeks before. The first child, who had not been vaccinated, got polio. It seemed obvious to me that the vaccine virus had reverted to wild-type polio virus in the vaccinated child, that it had passed through his intestine, and most likely via contaminated hands as they played together, polio was transmitted from the vaccinated to the non-vaccinated child. I reported that information to Dr. Langmuir at CDC. He went to Washington where he was on a committee with Sabin and others to evaluate vaccine-associated polio. Langmuir wanted to count my reported case as vaccine associated. Sabin did not want to count it—saying that his great vaccine shouldn't be blamed. Sabin won the argument, right after he had reported Cincinnati to be free of polio for two years because of his vaccine.

VH: Scientists don't like to have their great work blighted by "ugly little facts," as Thomas Huxley said.

PG: Right. Another interesting situation grew out of my investigation of an equine influenza, a infection of type A influenza virus in horses that we knew was going to come from South America to the horse tracks in the United States. I set up to study whether it would be transmitted from the horses to people. There's a trotting track south of Columbus, Ohio, named Sciota Downs; I went there and drew blood from the drivers of the two-wheeled sulkies pulled by horses and from the grooms. I took their blood, and I swabbed their noses for the virus. I also took blood from the horses, with a veterinarian's help. It turned out that some of the horses did get equine influenza, and many of the men got colds. If you just glanced at the situation, you might have thought that the horses and the people had the same virus, but this didn't turn out to be true. The men slept in the barns, in crowded conditions with men and horses, and probably exposed each other to whatever viruses were around. The horses, some which came from other tracks and some from nearby farms, showed transmission of the flu virus, but it didn't cross species to people.

The men involved with the horses got to know me, and when I said, "I want to learn to ride one of these things," a couple of them said, "Come out, 6 a.m., and we'll set you up with a sulky and a horse." This was fun for me. At 6 am I sat in the sulky with my feet up, big bicycle wheels on the side. The horses went in the wrong direction [the opposite direction from when they are racing], so the horses knew that they were just exercising. If they go the other way, they go full speed. Then the men started showing me their tricks. They'd lock their wheels with my wheels, they'd whip my horse, thinking it was funny.

The trotting people were very nice. They liked horses; they took good care of them. I also went to a track for galloping horses that was terrible. The owners and jockeys didn't seem to care a damn about their horses. If the horse had the flu, they'd put them in the race anyway because they needed a sufficient number of horses. I wasn't a bettor, but you could tell which horses had no chance to win because they had flu.

Then there was an outbreak of erythema infectiosum in Circleville, Ohio, a little town south of Columbus. It's also called "fifth disease" [from a nineteenth-century classification of childhood diseases associated with rashes] in which kids would get a blotchy rash and in kind of a lace pattern. The outbreak happened just about the time there was a new toy around called Flubber. Flubber was a rubbery ball thing that would bounce very high. I think it means flying rubber, because it would bounce way up.

People were speculating whether Flubber caused the children's rash. I just plotted the sales of Flubber and the onset dates of the rashes and found the rashes began before the Flubber was available in the stores. There was not a direct link. Then I also trapped some starlings because there were a lot of starling droppings around where some of the kids played. I brought samples of the droppings to a virologist at the Ohio Health Department lab, but they, too, turned out negative. The outbreak was finally diagnosed as fifth disease, and my work helped to characterize the age distribution and how it was spread.

VH: What you are describing is all very, very practical. Traditional CDC "shoe leather epidemiology" –

PG: Yes--figure it out, and then act on it. I think the follow-through is critically important for public health.

VH: This is also the period in which you handled a case of anthrax.

PG: Right, in one person.

VH: — that was documented in Berton Roueche's 24 April 1965 *New Yorker* article, "A Man Named Hoffman." Would you tell me about this incident?

PG: The first I knew about it, this fellow living between Dayton and Cincinnati had died of anthrax. Anthrax is very easy to diagnose if you see the bacteria. The Ohio Department of Health Lab service specimen said "anthrax," so I took off to find out what was going on. It turned out that the victim was an insulation worker, and he'd been working in a hospital putting in insulation, which looks like the matting you put under carpets. He was putting this around the pipes. He had carried the bales on his left shoulder. We hypothesized that anthrax spores from old goat hairs in the insulation were injected by sharp hairs into his neck and into his thoracic duct; then he got a very aggressive case of anthrax. The company denied this possibility. They said that all their material was bought in the United States. I said, "Let me check all your batches." The company had a storeroom with the batches of insulation labeled with numbers like 303, 315. One did not match, and I said, "Where did this come from?" They weren't sure. It turned out that the supplier had been short, so they brought a different batch that was imported from the Middle East, through New York and New Jersey. And so I thought, just from this brief study, that this batch was probably where the anthrax had come from. I pulled out some specimens from the insulation and brought them back to Columbus. I did that all in one day. It didn't take me that long. That incident happened just before I took off for the Tongan Islands and the smallpox vaccine work.

I never saw Roueche, but I wrote all this up in my report. And then Phil Brockman, who was an anthrax expert, came into the case. I thought he tried to get a lot of the glory by pretending that I was this young person who had never seen anthrax, but I'd seen anthrax before in Iran.

Brockman didn't help with uncovering the problem, but he did do a follow-up that was very useful. With the spores, he was able to prove that it was anthrax. Then he went to New Jersey, where they were importing the goat hair, and got the rules for treating imported goat hair changed. Men were using shaving brushes for shaving that put them at risk for anthrax if there were spores on the bristles. Brockman got the rules changed so importers had to ensure that the anthrax spores were killed. I felt that when Roueche wrote the story, there was a slight change that gave a little more authority to Brockman than he deserved, but he did deserve a lot of credit.

VH: I remember reading it and thinking, "Peter sounds almost like the hired help here."

PG: That was nonsense. I actually had it all written up, which tickled the Ohio Department of Health people because when Brockman arrived in Ohio, I handed him the completed report with the answer and the analysis. He thought he was going to come in to show us. So there was just a little feeling of one-upmanship that was mostly from him, I thought, because I was just a young epidemiologist.

VH: You were the young person, and you could step aside and let the older man take the credit.

PG: Yes.

VH: From 1964 to 1966, you did a residency at Tufts. Tell me about it and what kind of influence that might have had on your career.

PG: Okay. I was really on the Tuft's service of Boston City Hospital.

VH: Okay.

PG: Boston City Hospital had three services at that time: Harvard, Tufts, and Boston University. The three medical schools divvied up the wards with some training, cross training. For example, I took neurology with the Harvard group. The hospital generally served low-income people, like L.A. County, not quite as wild, but with very good academic training. I was taking training in internal medicine. I did a lot of cardiology-- more cardiology than oncology by far. I saw many patients with multiple, advanced medical conditions and learned how to deal with them. The head of internal medicine at Tufts was Fernando Baguria [Fernando Baguria, M.D.], an old time doctor. He was great at taking careful medical histories and doing physical exams (rather than depending on lab tests). The interns and residents learned not to look at lab results first but rather to give careful attention to the patient, what the patient was saying, what he looked like on a careful exam. That was good. In hematology, we were mentored by Jane DesForge [Jane DesForge, M.D.]. She also was the deputy editor of the *New England Journal*.

There were outstanding kidney people, Arnold Relman [Arnold S. Relman, M.D.] was one, who later gave me my oral internal medicine board exam, which was kind of odd because they let me do it at Boston City, and I was very familiar with the kind of patients there. On the boards, if you have to deal with a patient who has multiple systems disease, it's a little harder, but if you have a lot of experience with that, it's not hard. One thing that struck me was that all the other residents had prior straight medicine internships. I had a rotating internship at L. A. County. I think I was better off having experiences with surgery, obstetrics, orthopedics, etc., in addition to internal medicine rather than just going straight into internal medicine alone.

VH: I was also interested in the student health project with which you were involved at the University of Southern California. Tell me how this worked.

PG: Okay. I worked on that during two summers. After my two years of internal medicine training at Boston City Hospital, I had to decide if I wanted to be a chief resident, which meant learning to be an administrator, good if I wanted to join a medical school faculty. But I didn't want to do that. I planned to get a master's degree in public health and epidemiology at Harvard. But there was a time gap. The residency ended June 30th, and the master's program started in September, so I had the summer off and wanted to go rafting down the Colorado River. But again, my plans were changed by Paul Wehrle. He knew that I was going to be free and also that I had plans to go rafting.

The situation was this: Some medical students in California had, on their own, started a student health project, in which they went to areas with poor populations or joined community groups to see if they could do some good as young medical students. Paul was their faculty mentor, responsible for them, but he wanted me to come and actually do the supervision of them for him. So he said, "Why don't you forget about rafting? Spend your summer out here." I did. The first was summer in L.A., and the second was at Menlo Park, right next to Stanford. Some of the students would care for the migrant workers, some would be in Watts, some would be other places, helping run clinics or get patients in. The second year, especially, there were students in Haight-Ashbury because there was LSD [Lysergic acid diethylamide], and all the druggies were going there. I'd oversee that work. Students worked in East Palo Alto, which was the poor black area next to Stanford. And some work was also done on farms out in the Valley. The biggest problem in Haight Ashbury was hundreds of young people on drugs. But mostly they had colds and cuts on their feet from going barefoot. Some were being detoxified. The students had set up a room in Haight-Ashbury, with mattresses around it, and they'd just stay in the padded room with the kids being detoxified until they got over the drug.

VH: And it resulted in a publication.

PG: Yes. There was a USC published book with short articles by the students. But I don't know that it every really got published where you could retrieve it.

VH: In the fall of 1967 you went to Harvard to work on a master's of public health in epidemiology. Now this is something we've talked about before, but I'm going to ask you to explain it for the record here. You encountered a whole new way to think about epidemiology.

PG: Yes.

VH: The Harvard approach was very different from the CDC approach. Tell me about that.

PG: There were two giants in the field of epidemiology: Alex Langmuir, who built CDC from a sleepy malaria-control agency to a world-leader in the epidemiology of infectious disease, and Brian MacMahon [Brian MacMahon, M.D.], who was a very scholarly, evidence-based scientist in epidemiology at Harvard, who developed rigorous methods for chronic disease epidemiological research, especially cancer. I went into the Harvard program having had training in what Langmuir called "shoe leather epidemiology." You figure out what's going on; you figure out what to do about it; and you take responsibility to follow through. So when I arrived at Harvard, I went to Brian MacMahon and said, "I've been an EIS officer. Do I have to take the basic epidemiology course?" And he smiled—he was a very nice, quiet, a little bit introverted guy—and said, "You not only have to take it; you've been brainwashed. You have to be deprogrammed. CDC has a very good program. The only problem with it is that people come out thinking they know epidemiology."

So I signed up and took the introductory epidemiology course. He was right in the sense that I didn't know his approach to the subject, which was very analytical. How do you set up comparisons avoiding bias? How do you design case control studies? How do you do cohort studies? What are the statistical methods linked to that? How do you use big data sets? We had to learn computer programming at that time. MacMahon had written a textbook of epidemiology, which described his approach. For the epidemiologists you see at NIH now—among the best people in the country—a lot of them are essentially a Brian MacMahon alumni association. He trained a huge number of people.

VH: Such as?

PG: Joe Fraumeni [Joseph F. Fraumeni, Jr., M.D.], Bob Hoover [Robert N. Hoover, M.D.], Philip Cole [Philip Cole, M.D.], David Sackett [David L. Sackett, M.D.]. They're all over the place. If you go to a cancer meeting of epidemiologists, or certainly if you did 10 or 15 years ago, many of the heads of academic departments had gone through either the Harvard program or the program at Johns Hopkins with Abe Lilienfeld [Abraham M. Lilienfeld, M.D.]. They were the two big strong programs. And they didn't always agree with each other. During my entire time at Harvard School of Public Health, I also saw patients in internal medicine while on the staff of Peter Bent Brigham Hospital.

I also had close ties with Abe Lilienfeld, although I never trained at Hopkins. I don't exactly remember how it started, but I think it was when Harriet [Harriet Reif Greenwald, Peter Greenwald's wife] and I first moved to in New York State in 1968. I didn't know anything about NIH, but a fellow in Buffalo named Saxon Graham [Lloyd Saxon Graham, Ph.D.], a sociologist who was doing cancer epidemiology, said to me, "Why don't you apply for an NIH grant?" I hadn't ever applied for a grant. I was on MacMahon's teaching grant, I think, but I didn't know anything about it. They just paid me—a little. I got an NIH grant application, looked at it, and applied for this great big program project grant, a combination of several different studies, all sorts of things, such as tracking down Hodgkin's disease outbreaks. There was a site visit, which Abe Lilienfeld chaired. It was wonderful. The site visitors came and gave me an education about grant writing. They would say things like, "That's a good way to do this. Have you thought of modifying it this way?" I agreed with their constructive suggestions. As a result, I got a great score [from the NIH initial review group], and was given this great big grant.

When it came time for renewal, I was concerned that all of my colleagues whom I'd put on the grant were leaving me with the burden so I said, "We're not going to reapply. We're going to have multiple RO1's [investigator-initiated grants] out of phase (i.e., submit the proposals in different years so they were staggered). And so, by the time I came to NCI, I had what I thought was a small program in epidemiology, but it was about \$7 million a year from 12 different sources. When you've got that many sources, you can do whatever you want.

Abe Lilienfeld also helped me in a later study. I was looking at the pattern of brain tumors across upstate New York. In Monroe County, where Rochester is located, the incidence of tumors was higher than in the rest of the state. I went to Rochester to figure out what was going on and found that there were more brain tumors in the Eastman Kodak workers than in the rest of the population. Kodak also has a huge plant there. I linked up with the epidemiologist at Kodak, Barry Friedlander [B. R. Friedlander, M.D.] who is an outstanding occupational epidemiologist trained at Hopkins. We laid out a plan to do a joint study of brain tumors in Monroe County and at Kodak. And we decided that it would be smart to have an independent referee to make sure that the study was well done and that nobody could say, "company bias," or "government bias." The independent referee we chose was Abe Lilienfeld. We asked him, and he said sure.

What we found was that the high incidence of brain tumors was not in Kodak chemists, even though we had found all the old timers in order to figure out exposures. It was office workers and others who had the highest incidence. We had a positive control group of people from the community who also had brain tumors. The results of the study showed that the Kodak people tended to have better medical care. They'd have better diagnostic tests. This was before CT [computed tomography] scans, and we ended up thinking, "Probably, in the over-65 group, hidden in strokes and other symptoms, were some brain tumors that weren't found if someone went to a nursing home or was cared for elsewhere." It was just better medical practice that revealed the apparently higher incidence at Kodak. We published that, and Lilienfeld looked at it and thought the idea of a positive control was clever. He was very helpful and very friendly. Ever since, when I've go to some professional meetings, I get invited to the Hopkins receptions almost like I was an alumnus, but I never studied there. Hopkins tried to recruit me before I came to NIH.

Another interesting study that used both the New York State cancer registry and field epidemiology was my confirmation in 1971 that maternal use of the synthetic estrogen, DES [diethylstilbestrol], led to vaginal cancer in their daughters as teenagers and young adults.

VH: If I remember correctly, it was also during your time at Harvard that you met Harriet, and you two married in 1968. It's a wonderful story about how you met Harriet. Can you tell that story for this recording?

PG: In 1967, I was living in Boston and Harriet was living in Cambridge. One evening I was bored, so I went to a party in Cambridge. Harriet was at the party. She was not smoking when I met her, and I probably wouldn't have talked to her if she had been (she rarely smoked, except if she was at a party and bored) but we met at the party. Year's later, when our daughter Laura married Ian Frommer, I said that my being bored once in 1967 was a cause of Laura's marrying Ian. This was an example of chaos theory, and Ian's Ph.D. thesis advisor, James York [James A. York, Ph.D.]—an originator of chaos theory in mathematics—was in the audience.

VH: Harriet was a graduate student in Colonial American history at Harvard, if I recall correctly, working with Bernard Bailyn.

PG: Yes, she studied Colonial American history with excellent historians. Bernard Bailyn was one. I think she was a little overwhelmed when she came from UCLA to Harvard but was more than a match with them in brain power.

VH: She came at a time when very few women were graduate students in history departments.

PG: Yes. She certainly had the capability for it, but I think--

VH: Many male professors were not pleased to see a woman taking the place a man might have had.

PG: True.

VH: As I looked at your CV, it appeared to me that 1968 was a turning point in your career, when you began to shift from being trained to being a full-time leader. Would you view this as a point when you were shifting gears professionally?

PG: Yes. My choice then was whether or not to stay at Harvard, which MacMahon wanted. I was in a superb class at Harvard. There were six classmates who were very strong in MacMahon-type epidemiology. But Harvard looked like it would be a long career path; I'd just be an academic at Harvard. I was never that impressed with the glory of being at a prestigious university. I think other people might be, but I didn't care that much about it. I wanted to focus my career on linking research and public health impact.

VH: What did you really want to do?

PG: I wanted to go to a place where I could do both the Langmuir type of epidemiology and the MacMahon type, and have a public health impact. The people in the New York State Department of Health somehow knew about me and were telephoning me, and other people were recruiting. New York State looked like a situation where there was a state cancer registry, an old well-established one. I could step back and say, "Look, New York State, I can do these population studies"--the MacMahon type. I also could do things more or less like I'd done in Ohio when I was with CDC, where I could go out and figure out what's going on and could implement the necessary corrective actions. Also, the salary, which at that point was \$25,000 a year, was a good salary compared to Harvard.

So I decided, in spite of MacMahon, that I would go to New York State on my own and run a division of the health department.

VH: Let's take one more little side step here and talk about your philosophy of medicine, and what, for you, made medicine and being a doctor important as your life's work.

PG: I was coming more and more to the belief that I could be a good internist, but that there were lots and lots of good doctors, and I'd just be one more doctor seeing patients. I like to do clinical medicine, but I thought that public health could have a better public impact, and it was very thin in depth of good people. It wasn't valued as much because acute patient care gets more attention. So I was moving more and more toward public health, and particularly prevention.

Here is one example of the applications I did in New York State: We analyzed cervical cancer deaths and found that many of the women dying of cancer of the cervix had been in hospital emergency rooms within the two or three years before. They did not have a Pap test [Papanicolaou gynecological screening test] at the hospital, and their deaths might have been avoided if the testing had been done. I drafted a letter for the state health commissioner to send to all the hospitals in the state saying something like:

"From now on, if a woman comes in for any emergency treatment and has not has a Pap test done in three years, she gets a Pap test, unless there's a medical reason not to. And we're going to come around in six months, and check on it. If you're not doing it, you can be fined \$1,000 per woman who wasn't tested." When the six months was up, I wasn't sure what to do. I said to my staff, "Okay, we're all leaving Albany, and we're going to visit every hospital in New York State in a week. We're going to look at their records on Pap tests." Based on my analysis, I estimated that 75 deaths per year could be prevented if doctors did this.

What was interesting was our reception at these hospitals. It was very different than it was when we did our epidemiological studies. When we did epidemiological studies, we would get stuck in the corner near the records room clerk or something similar, and the hospital let you do what you wanted. Now we had the red carpets rolled out, the hospital's director would be there. The potential \$1000 fine got their attention. A lot of the hospitals had changed. The best hospitals for Pap screening had required the admissions form to include Pap tests done or not indicated, which a physician had to fill out before he could get paid for the hospitalization; or they had a doctor who was a medical leader, who was a strong personality in that hospital said, "We're going to do this," and made sure that they set up an arrangement. Often, the Catholic hospitals seemed to be the best, but that's just an impression. I felt the nuns running those hospitals were very dedicated, but I didn't really collect any data showing that.

VH: But the reward system for the hospital to be able to say, "We have lower death rates" must have been a great attraction.

PG: Yes. You would think so. That effort worked to benefit patients, but I never really had a long-time follow-up, so I can't say that it was beneficial in the long term. For a specific period, it certainly worked.

VH: When you first went to New York State, I believe that your job description, and maybe the title, was broader than just cancer epidemiology, correct?

PG: It started with cancer. Initially, there was a cancer registry, and not a lot else, but then I built it into a research and applied program. I then became director of epidemiology, which included cancer, infectious diseases--like a mini CDC--heart disease, and a little neurologic disease.

VH: I see. So it went the other way then. It started more narrowly, and became broader as time went on.

PG: Yes.

VH: One of your early studies was an Atomic Energy Commission monograph, *The relation of radioactive fallout to leukemia and fetal mortality: a reconsideration*. Tell me about that study.

PG: It started with a physicist named Ernest Sternglass [Ernest J. Sternglass, Ph.D.] at Pittsburgh [University of Pittsburgh], who, in my view, was a bit of an alarmist about radiation. Not that radiation isn't a problem, but there were always these intellectual camps in which some people would push it beyond the evidence, being very selective in what they chose to highlight. Sternglass had identified an atomic bomb test--I think it was in New Mexico, in 1953--in which some of the radiation blew over the Albany-Troy area in New York. The RPI [Rensselaer Polytechnic Institute] people documented that there was some fallout. But Sternglass claimed that there was more leukemia in Albany in that period and more birth defects that were due to the radiation. So with the health department statistician, Sandra Kinch, I studied it. She helped me analyze it. We pulled all the cases referenced by Sternglass, and his assertions fell apart.

I don't remember exactly, but I think there were five leukemia patients. One woman had had a Cesarean section. We couldn't tell if she had x-rays before the operation, but it seemed possible. There were two babies who were not born the year he claimed, or not born in that area, but he was attributing their deaths to prenatal exposure from the fallout. This was impossible, because they weren't there, or it was the wrong time. We ended up with the same or fewer cases than would be expected at the normal occurrence rate. Regarding fetal deaths, all we did was separate New York City's incidence from the incidence in upstate, and all of the excess was in New York City.

Sternglass was talking about Albany, so it was just nonsense. And we published it.

VH: A number of these studies that you did sometimes confirmed what was causing cancers and other times . . .

PG: Refuting it.

VH: You were refuting the fear. This has been one task of public health epidemiologists, which doesn't always make them popular.

PG: That's true. I think, unfortunately, now there is such an army of epidemiologists and other scientists, whose aim is publish, publish, publish, and there are so many journals, that there's a lot of flimsy work as well as rigorous work. You end up with people having to spend time restudying things that were not well studied. Also there can be either false alarms or a false sense of safety. Often the false alarms are picked up by the press. If the press picks it up, you have an issue. A recent concern like this was the possible influence of cell phones on the development of brain tumors. The evidence is against an association.

VH: And high energy lines too.

PG: Yes. High energy lines are tougher to study. Once when I was in New York State, the power companies wanted us to study high energy lines. The reason was it cost them a lot of money because they needed to purchase wider right-of-way and put the poles higher for electric power lines because of health concerns. The companies wondered if they could make the right of way narrower and the towers lower. I looked at it and decided that there were not enough people living close enough to get a reasonable measurement and set up a decent epidemiologic study. That is, the electrical fields from the power lines were narrow and largely went through farms and other rural areas. On top of that, one of the questions had to do with circadian rhythms and sleep patterns, and there's no way to get a real handle on what was going on, so I just said, "Sorry, study some mice if you want, but we're not going to study it," because we didn't have a sound way of doing the study.

VH: In October 1968, your title was Director of Cancer Control, a position you held for 10 years. Would you please explain to me now, for the record, what the difference between cancer control and cancer prevention is?

PG: Cancer control was set into law in 1973 and linked to the National Cancer Act of 1971. It was interpreted to mean actions of direct benefit to people, but not research. Cancer prevention was just one aspect. Several outstanding people like Lester Breslow [Lester Breslow, M.D.], a public health physician from California, pushed for inclusion of cancer control as part of the National Cancer Act. Lester was head of the Health Department for California and later Dean of the School of Public Health at UCLA. He's still very active at age 92 or so. Cancer control was defined into law because of a concern by Congress that NCI would never do things that helped the public in the short term. NCI would just do research under the National Cancer Act. So Congress set up a sequence of \$10, \$20,

\$30 million over three years to do cancer control. But Congress didn't define it that well either, except that it was supposed to be some sort of community activity to benefit the public. During the decade of the 70s, here at NCI cancer control was interpreted as defining "out" any research (i.e., research could not be supported with cancer control funds). Administrators would sit around the table and say, "What should we do?" and give money to specific things, often public education efforts.

VH: Stop smoking efforts? That sort of thing?

PG: Yes, and screening, and improving access to care, and addressing disparities. What they did wasn't soundly anchored in research to see if it was effective, or efficient, or the best way to go about it. And that's what I changed when I came here. I never accepted that research should be separate from control. I always felt you link basic research to clinical research to research on applications and public health. You use research on effectiveness to aim for an impact, a benefit to large, defined populations.

VH: But cancer control at that time was not giving money for things like radiation implants for prostate cancer, was it?

PG: No. What happened here at NCI was that NCI leaders would want money for something, and they'd say, "That's cancer control, so we're going to define it in." And the big thing defined in was "rehabilitation," which some surgeons championed. So for a while, NCI leaders were saying "cancer control and rehabilitation," and using the cancer control funds for that. The funds were sheltered as a separate item in the NCI budget. This became very muddled under Klausner [Richard Klausner, M.D., NCI director, 1995-2001], as he didn't want to be constrained to use the money for cancer control. Also, NIH couldn't see why NCI should have a special category when no other institute does.

VH: In this period then, cancer control was a vaguely-defined –

PG: It was vague.

VH: Was cancer prevention talked about separately?

PG: At NCI there was a Division of Cancer Etiology and Prevention before I came in 1981. I got that changed. I'll tell you why. I wanted prevention in my division.

At that time, prevention was, to me, falsely defined as anything having to do with studying causality, etiology, and epidemiology, with nothing that involved intervening to lower the occurrence of cancer. To me, etiology is to *know* something; prevention is to *do* something to bring down risk. At NCI, prevention missed the do-something part.

Scientists were lying, I thought, when they said "prevention" because they weren't doing anything to lower the risk in people. They were just trying to find out cause. They would say, "Until we know the mechanisms that cause cancer," which could take many, many years, "we're not going to do prevention." That was an excuse to do just research on causality. Research on causality is important, but it's not prevention. They're linked, but you have to intervene for it to be prevention. That was my view.

VH: That is very important, and I'm glad to have you explain the difference so clearly, because I was confused. Jumping back to your time in New York, one of the first articles that you published in New York had to do with the need for stricter legislation to control tobacco and cigarette advertising in response to the expiration of the health warning label on cigarette packages, which was going to occur July 1st, 1969. I found it very interesting in reading the papers about the different involvement of the Federal Trade Commission and the Food and Drug Administration and the anti-tobacco lobby and the pro-tobacco lobby at this time. Tell me about this from your perspective in New York.

PG: Okay. New York was a little different than when I got to NIH, so I can explain them both. Basically, it was obvious by that time that the predominant cause of lung cancer and several other cancers was tobacco use. We did what we could do in public education. The first thing I did while I was still in Boston, knowing I was going to New York, was to address the tobacco problem. I started a project with Dorothy Nelson, a health educator at the New York health department to get spouses, mostly wives, of doctors all over the state, to do an education program and get them involved in anti-tobacco efforts in their communities. Our feeling was at that point that the federal government was never going to do anything about this. It was so controlled by tobacco lobbyists that there would not be meaningful policy legislation, and that it was better to do public education at the state and community level. Also, I was against the so-called safer cigarette, because I thought it was a fraud. Even then, it was known from Wynder's [Ernst L. Wynder, M.D.] group and others that you could put a filter in a cigarette, but because of the way the holes were put at the side of the filters, people would get just as much tobacco, inhaling more or covering the holes with their lips.

VH: In one of your articles, you talked about the importance of physicians' giving forceful advice to their patients to stop smoking.

PG: Right.

VH: What could physicians do in 1971 to help patients quit, beyond urging them?

PG: The main things were pretty simple. One, ask people if they want to quit. Even then, many people wanted to quit. In fact, the peak drop in smoking started in the '60s, even though the change in lung cancer rates didn't come until later. If you just said, "Have you thought about not smoking?" a lot of people would say "yes" but have trouble doing it.

Then give them a strong message. "As your physician, I have to advise you, you should stop smoking. It's bad for your health for many reasons." And then follow up. Have your nurse call back in a few days, or say to the patient, "Next time you come in, I want you to tell me what you've done. I'm going to write it in your record that we talked about it." I think the main thing was to say, "As your physician, I feel it's important. If you have an interest, you should take action." There were things, more or less like what I'm saying that got more codified over time. I'm not sure I'm saying it exactly the way I said it at that time, but it's more or less along those lines.

VH: I guess what I'm getting at is that in the 1970s, smoking was not considered an addiction and there were no methods such as the chewing gum or the patches available to help people quit.

PG: What I have described is only one part. The other part was that the physicians treating cancer patients for most of the counties and cities in New York State were the ones leading local American Cancer Society activities. I would encourage physicians as community leaders, and I'd work with the Cancer Society to do this. I would say, "Speak out, get your school system working on this. You're a community leader, people look up to you as a doctor, take some responsibility." And some did. I mean, some were quite good, but lots of others said, "It's not my role."

VH: You studied many different types of cancers over the course of your years in New York. There was a most interesting study in 1971, papers describing what looked like a cluster of Hodgkin's disease. Now I remembered that there had been at least a suggestion that Epstein Barr Virus was connected to Hodgkin's, but it appears that you didn't find this. Tell me about this whole thing.

PG: We were looking at different potential connections. It was rare for children under age 10 to get Hodgkin's. We thought it could be immunity issue. About age 10 was the time that tonsils would shrink, so we thought that perhaps tonsillectomy was related to Hodgkin's. I don't now if it's true or not. It looked that way as an association. There was a group at Yale studying EB Virus--Evans [Alfred S. Evans, M.D.] was the virologist—but they couldn't prove an association. We also had a large cluster of cases comprised of students from a class of Albany High School. The incidence of Hodgkin's was not only in the class. If you did a network of acquaintances, you got a pattern a bit reminiscent of infectious disease spread. We thought it might be more or less like polio, where several hundred people might get exposed, but only one would become ill—Hodgkin's would be a rare manifestation of a common exposure of some virus. I was actually seeing patients with the oncologists at Albany Medical School. I'd help with therapy, but mainly, I was trying to culture viruses out of their throats and from their close relatives. However, this was unsuccessful.

VH: This was the peak time of looking for viral connections to cancer, wasn't it?

PG: Yes, it was. Even before that with Huebner [Robert J. Huebner, M.D.]. Huebner was very active in this period and the leader in looking for viruses at NIH. I met him at USC [University of Southern California] when I was invited there to discuss our work. One of our publications about the school cluster had interested them. They wanted to know, "Who is this guy up in Albany, and what's the evidence?" So Brian Henderson [Brian E. Henderson, M.D.], who was at USC then and had worked at CDC, and I think with Huebner--I'm not sure--but now is Dean at USC Medical School, invited me out to present what we were doing. Our problem, in retrospect, was that I'd culture the throats of patients and their family members and bring the cultures back to our lab. But at that time, the New York State Health Department Lab, which is good quality, only had competency for DNA [deoxyribonucleic] viruses, not the RNA [ribonucleic] viruses--the methods hadn't been developed yet. Even now if you look at, say, hepatitis C, which is caused by an RNA virus, we don't have the vaccine yet. Hepatitis B, which is caused by a DNA virus--easier problem. There is a vaccine. So we looked and looked, but we didn't isolate any viruses. All we had were patterns that were suggestive of associations, but we couldn't really confirm them.

VH: The shift to molecular biological methods was just beginning at that point. In 1974, you finished up your doctoral dissertation with the dissertation on the epidemiology of prostate cancer. Did this research teach you any particular thing that we should know?

PG: I was using the methods MacMahon championed. Basically, my thesis was made up of two studies. I could've probably finished a lot earlier, but I already was an M.D., so there wasn't any rush to get my Dr. P.H. I brought the studies back with me to New York State and was working on the thesis while I was doing that job. Finally, I had some other people on our staff work with me just to get it completed. One study was a retrospective cohort study of men with enlarged prostates to see if that predisposed to prostate cancer. I was studying men from the Lahey Clinic in Boston, who in the past had been treated for obstructive symptoms from benign prostatic hypertrophy. I got their old records, and then I traced them to the present and compared them to matched controls from the Lahey Clinic. The study did not show an association. One of the touchiest episodes in conducting the study was that the Lahey Clinic called and asked me, "What do you want this record for?" I said, "I'm doing this study." "Well, why did you want *this* record? It's locked up." They were sensitive about it because it was Joe Kennedy's record [Joseph P. Kennedy, Sr., former U.S. Ambassador to the United Kingdom].

The second study was an attempt to learn if physical features earlier in life could be used as a predictor for prostate cancer. It was known, for example, that with breast cancer, age at first childbirth was known to affect the later incidence of breast cancer.

But we had no way of getting a handle on that with prostate cancer, so I wanted to look at physical features, hair pattern, maleness patterns. I worked with an anthropologist named Albert Damon [Albert Damon, Ph.D., M.D.], a Harvard anthropologist, who was working with a whole data set from the early 1900's of Harvard grads. He also had data for graduates of the University of Pennsylvania, but I only worked with the data from Harvard. Anthropologists had examined all the Harvard students--all men--with nude photos. They had ranked them with somatotype scales and scales of androgyny--different scales of maleness and femaleness. They had information on patterns of hair, hair on the back, hair between the pubic area and the belly button--all these kinds of things. I wanted to see if any of that would relate to prostate cancer. We also had the 25th anniversary pictures showing baldness. I wondered whether there might be something going on during the teenage years, or the 20s or so on, that might give us a hint about future incidence of prostate cancer, because at that time we had no other way to get a handle on hormonally induced features years before diagnosis. The study was carefully done, but it didn't show any positive connections.

VH: Well, what I'm seeing is the pattern over this period in New York in your career of all these different experiences that gave you a great depth of experience with a lot of cancers.

PG: It was wide in scope. And people at NCI don't know it, but I also had a lot of experience in infectious diseases. I was overseeing it all, including heart disease and even multiple sclerosis studies.

VH: But one of the things that struck me was that you wrote a chapter in a book that was apparently a tribute to Edgar Sydenstricker, a pioneering statistical epidemiologist here at the NIH. What drew you to Sydenstricker? How did you end up writing that article?

PG: In 1973 and 1974, the Milbank Memorial Fund wanted to sponsor a book dedicated to Sydenstricker's life. Sydenstricker had worked in accounting in upstate New York near Binghamton, and that's probably why they got in touch with me. I agreed to write a commentary for the book, which they called *The Challenge of Facts: Selected Public Health Papers by Edgar Sydenstricker*. Another doctor at the New York health department, Robert Korns [Robert F. Korn, M.D.], had been born to a missionary in China and had met the author Pearl Buck. He also knew about Sydenstricker, and he wanted to co-author the commentary with me. I said, "Sure." We looked at Cattaraugus County, New York, where there had been TB [tuberculosis] control programs that Sydenstricker had developed and published, about 1925, where what was known about TB control was instituted. It was viewed as a kind of public health engineering. Robert Korns, actually took the lead in writing this commentary, which I was happy about.

We compared the incidence of TB in Cattaraugus County to that in other counties from 1925 to 1936, to see and whether the TB control programs instituted by Sydenstricker had really made a difference. And even though Sydenstricker's programs were state of the art for that time, it turned out that they didn't make a difference. The overall time trend of TB going down just masked any possible benefit. TB was declining everywhere, and you couldn't say his program had specifically made a difference, even though it was well done. This was something Sydenstricker had worked on. As we wrote about him, we highlighted that story because nobody had ever actually done a follow up from that period. We did it through records at the New York State Department of Health.

VH: In 1978, you won the Redway Medal, an award for medical writing given by the *New York State Journal of Medicine*. Tell me about what you did to win this award.

PG: There was an epidemic of cancer of the endometrium of the uterus during the 1970s that was due to estrogen-supplement use. We had shown this pattern and wrote a story about it in the *New York State Journal of Medicine*. I tended to publish in that journal because it reached all the licensed doctors in New York State, and they were my target audience. I wasn't looking for *New England Journal* as a famous place to publish. I was looking at "How am I going to reach the doctors in New York State?" The award came out of the blue. I didn't know there was a Redway Award. I still don't know exactly why it was awarded to us. The article was practical because it had to do with effects of hormone use, and contributed to doctors' switching to a lower dose, duration, and use of estrogens, resulting in an end to the endometrial cancer epidemic. The old dose was arbitrary and way above that needed for an effect.

VH: As we come to the end of today's session, I noted that in the 1970s you were getting somewhat involved with NIH because you sat on a study section [an NIH initial review group for the grants program] from '74 to '78. And in 1979, the Surgeon General published the first book to provide nutrition advice, which becomes one of your fields. And finally, it was sometime around this point in 1979 when NCI director Arthur Upton approached you about coming down here. Would you tell me about all this?

PG: First of all, being on the study section of Epidemiology and Disease Control was a wonderful experience. We'd look at grants, and we'd do site visits. Some the time we'd try to be constructive towards promising people, not just knock them. You really learned about what it takes to write a good grant application, how they're evaluated, and you looked at things you might not have looked at that someone was working on. I think that kind of training is good for young scientists. I really valued the experience I got during those four years.

Then to Art Upton. Art Upton is a wonderful person, very kind, and an outstanding scientist. I think he was uncomfortable being NCI director. He was a highly respected radiation biologist, but he didn't like to be the one to tell people no, or sorry, or stand up to Congress when Congressmen were grandstanding on something that he knew was phony. So he just didn't like making tough decisions that were unpleasant. He had been in New York State, connected with NYU [New York University] in labs NYU had in Orange County.

Art Upton invited me to come to NCI to shape up cancer control and prevention. I talked to him for an entire afternoon--maybe three or four hours--about what I thought we ought to do, and he was very pleasant. He wanted me to come to NCI, but I never got the sense that he had the strength of management leadership to let me do what I felt I needed to do to turn things around here (drop some staff, hire others, and change the organizational structure). One thing that actually annoyed me was that he had set up a committee to look at what should be done in cancer control. Bob Hoover [Robert N. Hoover, M.D.] chaired it or co-chaired it. That committee had taken everything at NCI that nobody wanted, and the people that nobody wanted, and put them in a division with a really bureaucratic name, the Division of Resources, Centers and Community Activities, everyone called DRCCA (pronounced dra-ka). That was the division Upton wanted me to run. I might've considered it if I had known that I would have the backing, not in intent, but in the resource allocation and decision making power that would let me change things. But since I didn't, I declined. I just didn't feel that I would be able to do the job, even though I liked the opportunity.

Upton left NCI and Vince DeVita [Vincent T. DeVita, Jr., M.D., NCI director 1980-1988] started as NCI director shortly before I came. He'd been head of NCI's Division of Cancer Treatment. From Vince I got a very different picture. I had the picture of a strong manager who had no problem saying, "Here's what I think, and we're going to do it." So I went over things with Vince. It was clear, I thought, that he's going to back me up and let me make changes. And he did. When I came to NCI, for example, we had a staff of about 100. I pared it down to 40--I'd call people in and say, "What are you doing? What do you expect to accomplish over the next year? Let's spell it out.

You're accountable for that." Some would find another position and some would retire.

Then there was an issue over the name. I said, "I want the name changed. I want it to be called Division of Cancer Prevention and Control." Well, there was a "Division of Etiology and Prevention" that Dick Adamson [Richard Adamson, M.D.] headed, and NCI couldn't have two divisions with the word "prevention" in them. We went through debates, including at NCI Executive Committee retreats. Vince forced Dick to change the name of his division to the "Division of Cancer Etiology," which was more accurate. And Dick was okay with that. We're still good friends. I got what I wanted, and then we built up the cancer prevention program. We went up to about 200 staff and a strong program. We put science into it, which was a big thing. And how we did that probably should be part of the story.

VH: That's where we're going to start the next time. I will say thank you at this point, and I look forward to our next interview.